Saunders, J. Paul 1998

Dr. J. Paul Saunders Oral History 1998

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National Cancer Institute Oral History Project

Interview with J. Palmer Saunders

Conducted on July 22, 1998, by Gretchen A. Case

at Dr. Saunders' home in Perry Hall, Maryland

GC: This is Gretchen Case, talking with Dr. Palmer Saunders. Today is July 22, 1998. We are at his home in Perry Hall, Maryland. It is about 10:00 am. Okay. I don't know if you want to start by just telling a little bit about your background and your education and what brought you to NIH.

PS: Well, it's a long story.

GC: Okay. I have lots of tape.

PS: I was in the military and was recalled to active duty in the Korean War and through a set of advantageous circumstances, I met a general who had been my previous commanding officer in World War II. He asked me where I was headed. He saw that I had all my new uniforms there, post World War II uniforms. So I told him. I said, "I've been recalled to active duty and I have orders for Japan, the Tokyo General Laboratory," because I was a Medical Service Corps officer. He said, "Do you want to go there?" I said, "Well, I have orders." He said, "Well, wouldn't you like to go back to Edgewood [Arsenal]?" I said, "Yes, that would be very nice." He said, "Let me see your orders." He looked at them himself. "I'll take them. We'll get those changed."

And so in a few days I had new orders assigning me to Edgewood, the Chemical Corps Medical Laboratories in Edgewood, Maryland. That's where I spent the Korean War and did some civilian duty there, too, which I had done before, and that's where I became a *working* pharmacologist rather than a school pharmacologist. Learned a lot about toxicology and, in fact, I'm probably more of a toxicologist than a pharmacologist. People think pharmacologists are pharmacists and I always try to introduce myself by saying I'm a toxicologist.

So, after several years there, some of the people who had worked at the medical laboratories had gone over to NIH to begin in a new branch that they had, Research Grants. It became the Research Grants Branch, which in those days was equivalent to a division.

I was invited by the Director of that Branch, Ernest Allen—very famous, you've probably heard of him, in the Extramural Program—and I moved to become the Executive Secretary of the Pharmacology Study Section, which was chaired by Alfred Gilman, of Goodman and Gilman fame, and consisted of prominent scientists in pharmacology, most of whom I had never met. I was very flattered to work with him, and with these renowned people.

I did that and gradually after a few years I moved to other study sections to take them over and get them started. I introduced toxicology as a word there. Few people had ever heard of it as a scientific discipline and I was instrumental in starting the Toxicology Study Section, based on Gilman's thoughts about toxicology. He said one day—I had some toxicology applications to be reviewed in the Section—he said, "You know, Palmer," he said, "these won't go." "Well, why not?" He said, "Well, this is toxicology. That's just a *minor* branch of pharmacology. If you want to have these reviewed, why don't you create a new study section called toxicology?" So, we did.

So we created a new section and invited some leading toxicologists to become members. That really throve and there was a new society formed, the Society of Toxicology, and I was a charter member of that, having been instrumental in getting it started. I'm still a member actually, retired, but still charter, and we started it.

Afterward from there, I soon became promoted to be a Review and Assignment Officer; that is, my duties were to assign the applications to the various appropriate Study Sections. You needed a good staff to do that.

Then I became for a brief time Associate Director of the National Institute of Allergy and Infectious Diseases, because I had become interested in penicillin which, at that time, was a contaminant of the milk sheds, especially around Philadelphia. I knew some of the pharmacologists *cum* toxicologists in Philadelphia who were originally studying this problem, so I invited them to submit applications for this and get them reviewed and they did.

After about a year, there were some political issues in Allergy and Infectious Diseases that weren't pleasing to me and I was invited by Ernest Allen to come back to the Division, the Research Grants Review Branch, and become, once more, an Executive Secretary there; so I did. Eventually, I became Deputy Director of the Division. Then I ran afoul of the new Director that had been invited to replace Ernest Allen and his lieutenants. I didn't get along too well with him. So, I began to make my unhappiness known to various people in other Institutes.

The Director of the National Cancer Institute at that time was Ken Endicott, who I had known well, because he had been the Director of the Chemotherapy Research Branch of the National Cancer Institute. They are the people who operated chemical programs and were investigating many chemicals and new plant products to test their activities against cancer. So he invited me over to be a Deputy under Gordon Zubrod. Gordon Zubrod was a famous clinical oncologist who operated many sophisticated clinical testing programs. He is retired from the University of Miami in Florida and has recently died.

I worked with him for quite a while. Dr. Jim Shannon then asked me if I would lead a committee to investigate the Chemotherapy Program. One of the congressmen, whose name I have forgotten—maybe he should be nameless—got very upset about the money that the Cancer Institute was spending and he decided that the Cancer Chemotherapy Program was one of the big offenders in spending money. I don't know whether it was an offender, but it *was* spending money. So Shannon had been asked to look into that. He asked me to become the Executive Secretary of that extramural committee, chaired by Arthur Richardson, who was head of pharmacology at Emory University in Atlanta and an old friend of Jim Shannon.

So, we did that review. It took about six months. We visited drug companies who provided new drugs for the NCI to test and learned all the places that had contracts— contracts, imagine—with the NCI to do all the testing and evaluating. I began to wonder, why did we have them on contract? Why not have them as grants? Well, I'm going to tell you about that story later on. We reviewed the program and actually we found it wasn't a bad program. There were some problems with it, some deficiencies, especially in terms of review—that was the one thing we hit on. And we wrote the report. As the Executive Secretary, I had to write the report and get that approved by the committee, and that went to Shannon. Of course, I found out later that Shannon was not satisfied with the report, because he thought the Chemotherapy Program needed more change. He seemed to be quite opposed to it.

GC: Really!?

PS: I don't know why, but he was. Maybe this was in response to congressional pressure. I don't know. So, they weren't very happy with the report that said it was a pretty good program. Nevertheless, it stuck. Of course, the NCI was very happy because, while they recognized the Chemotherapy Program needed a lot of work, it was really a mainstay of the development of new cancer drugs.

GC: Right.

PS: So, that was okay. And about, I guess about a month after I finished that report, Ken Endicott came into my office one day and said, "Our Director of the Extramural Programs wants to go back to being a belly doctor." Ken Endicott was a character. He used terminology like that all the time. And he said, "Would you like to lead the program?" I said, "Well, let me think about that for a few minutes." I said, "Okay, I will!" I didn't hesitate, because it was back in my own field of extramural programs.

We had, oh, we were a *very* large program—about, I don't know, about a hundred and fifty people working in it. I recruited many scientist-administrators to work in the various fields as they were assigned to various study sections and they were the ones that sat in at the meetings and reported back to the Council. If they had any comments at the Council meeting, after they had been reviewed favorably or unfavorably by the Study Section, they would present them. So that was that.

So, that's how I started at NCI. I became a Division Director because the National Cancer Act of 1971 created two separate institutes. I don't know if they were called institutes. There was the National Cancer Institute and I think the Heart Institute was the other one. Two big institutes. And they were given institutional status and in the Civil Service, of course, titles mean a lot. That meant that all branch chiefs and associate directors became division directors. So I became the Division Director of this new division, which we had called the Division of Cancer Research, Resources and Centers. I think you'll find that information in that documentation I gave you.

We went on from there and then, of course, because of the 1971 legislation which President Nixon had signed in December of 1971, that became what they called the cornerstone of NCI. We can talk about that later, because I think there was an important problem there. So anyway, it became very political.

GC: After the 1971 Act?

PS: From 1971 on. And, of course, as you know, they did away with the old National Advisory Cancer Council and got a new body called the National almost a similar name.

GC: National Cancer Advisory Board?

PS: Advisory Board, yes. And then they created a three-man advisory committee to the President on cancer policy.

GC: The President's Cancer Panel?

PS: Yes. It became *very* political and, because of the constant conflict between the Extramural Program and the Intramural Program, it became more troublesome. One of my committees was chaired by a man by the name of Bill Levin, who was at the University of Texas Medical Branch in Galveston. He approached me one day and said that the University was looking for a Dean of the Graduate School. The former Dean had retired and the school wanted to have somebody and he thought that I would make a good Dean.

So, my wife and I hemmed and hawed about that for a long time and, I'll tell you, that was not a popular decision either way. It was a *very* difficult decision. After all, being in Maryland for a long time, I looked on Maryland as home, and the idea of going down to the "wilds" of Texas didn't really appeal to me. But anyway, they invited me down and I went down there and interviewed all the members of the search committee. This continued for about a year, and finally they invited me to bring my wife down. She agreed to come down. My daughter *reluctantly* agreed to come down; she was very much opposed to this idea. So, we all went down and they wined and dined us royally, as universities do, especially in Texas. They put us up at a big fancy hotel in town. It's no longer so fancy, but it was then and it was incredible.

So I went back. I had talked to the then-President, Truman Blocker of Texas City fame—the man who went in right after the explosion of 1948. I have to check that date; I'm not sure of that. A big explosion that killed a tremendous number of people, about three hundred. Two ships loaded with ammonium nitrate exploded at the dock and one of them exploded after the first one. Every fireman in Texas City and many neighboring communities were fighting this fire at the time that the second ship exploded and it killed them all. The burns, the terrible burns that they had. So Truman Blocker, who was a surgeon and had done a lot of burn surgery in World War II, because he was on MacArthur's staff, pitched in, along with several surgeons from UTMB, and took care of them. They have a lot of things named after him over in Texas City, but for various reasons not much is remembered about him in Galveston.

So, he had served as President for a five-year period and he was up for renewal. Of course, everybody thought that was an automatic thing. Unfortunately, he apparently had a political enemy on the Texas Board of Regents and they refused to renew his appointment. Of course, if one member of the Board objects, the other members usually go along with that; they like to be unanimous. And Dr. Blocker didn't have enough political strength with the other members to fight that, so he was out. When I heard that, I really had a lot of second thoughts, because I made all my commitments to Blocker and I had even written a letter agreeing to come. But I was kind of second guessing that one.

So Dr. Levin, who was my friend, having been one of the first who invited me to become a candidate, came up and met with me one day. We just chatted. He said, "When are you supposed to come down?" I said, "Well, originally I had told Dr. Blocker that I'd come down on July 1, 1974." And he said, "Dr. Blocker will no longer be President at that time." I said, "I understand that, so that's the reason I've decided to reconsider this and stay here." He didn't say anything for a while. He came back later to my office and said to me, "Palmer," he said, "I don't want this to go any further, but I have it on *excell ent* advice that I might be selected as the new President. So, will that make any difference in your decision?" I said, "Well, yes, it does put a different light on it, Bill. I'll be glad to reconsider that." So I did.

Marge and my daughter were getting very negative about this, because of the uncertainty, but because of Bill Levin's word, I decided I'd take a chance and we came down here on July the first.

At that time the University was set up with the Medical School which, of course, was the kingpin in the University, and the other schools were the School of Allied Health Sciences, the School of Nursing, and the Graduate School. The Deans of each of these schools reported to Dr. Levin, so I was directly, with the other Deans, reporting directly to Levin. I had been there about four or five months when I discovered that the Medical School controlled our budget: I had no say in the budget of the Graduate School. So, I went up to Levin and started to complain about that. I said, "Look, what kind of a Dean am I if I don't even have a budget?" So he said, "Oh, we'll take care of that." So, they took care of it all right. What they did was they gave me a *very* small budget, just enough to get by on, but with no extra money for development or anything like that. I was getting very, very upset about that.

So I decided, either I go back to NCI, if that were an option—and I'll tell you about that, too. People knew that I was hesitating and decided they'd recruit me as Dean of the Graduate School of Colorado University, the University of Colorado in Boulder, and various cancer centers wanted me to come as Director.

Well, I looked at a number of them and there were political ties in all of them which I didn't quite like. None of them were really successful. One that I had really decided I would go to was as Director of the Cancer Center at the University of Missouri in Columbia, Missouri. I took my wife that time, and looked at it. It became apparent that there was a political tie-in and so I really didn't have much control over that budget either. After UTMB, I decided that I didn't want to step into that mess again, so I turned it down and decided to stay. I told Levin.

By that time Levin was getting pretty annoyed with me for this back and forth, but anyway he said, "That's great." Then about three years later he made a decision, which he didn't consult me about, that the Dean of the Medical School would become the Vice-President for Academic Affairs, a new post, and in that post, all the Deans would report to him. So the Deans of the other schools would report to the Dean of the Medical School. I said, "Well, that's not very good. That's not what the commitment was that you made to me when I first came here." He said, "Well, that's right, Palmer." He said, "Okay. We're making an exception with you. We'll retain you as a member of the University Executive Committee and you will report to me." Well, that was a little bit of a legerdemain and didn't really work. I finished up my term.

About that time Levin had done ten years, I had been there ten years in 1987, and retired. I went into storage, as far as UTMB was concerned. I didn't have anything to do after that. They had a new President, Thomas James, who replaced Levin. He didn't want to know anything I did. I asked him, I went hat in hand to him and I said, "Please, give me something to do. I'm an administrator and I know a lot about UTMB, I know a lot about research." I even said, "Well, why don't you create a Vice-President for research and give me the responsibility for it?" No, he didn't want to do that. That would offend the Dean of the Medical School. "Well," I said, "you're offending me." "Why, you're not in it any more. You're retired." So, it didn't work. So, that's been the story of my life, I mean, that part of the story. Okay. And now back to NIH?

GC: Yes.

PS: You heard me mention extramural and intramural.

GC: Yes.

PS: Well, there has been a constant battle at NCI and at the other Institutes of NIH between the extramural programs and the intramural programs.

[Interruption]

GC: Let's just start with the intramural and extramural. You said we needed to go back and talk about it.

PS: Yes. Will you be able to pick up some of this in case I don't record it exactly the way it was?

GC: Yes, I think so.

PS: I'll try to remember how it was as I go along.

GC: Okay.

PS: The fight between Extramural and Intramural was long-standing. It goes back actually to World War II days, when Dr. Dyer, Director of NIH, was a member of the Office of Science Research and Development. They had some money left over after the war and they debated what to do with it. Dyer suggested that NIH would take care of the money and allocate it to investigators, just as the Board did during the war, and so apparently, with the Office of Naval Research, all agreed to that idea. Dr. Dyer laid down a set of rules and one of the rules was that extramural research should be strictly separated from intramural research; so that was the cardinal rule that we operated on.

Well, as I say, there was a conflict between Extramural and Intramural, because the research of intramural scientists was never reviewed by external scientists. It also had less money than the Extramural Program, but they were independently funded by the NIH budget and never had to suffer the kind of reviews that the University scientists had and have the chances of maybe having their application denied and their work stopped. So they didn't have to go through that. To some, it became evident that some of their research had fallen behind and was inferior. Certain laboratories were run by people who would come down from good institutions and they maintained them very well, but there were a lot of them that didn't.

One of the programs that we were concerned about was the Contract Programs at NCI, the cancer contracts, and we found out that a lot of those programs were not only not reviewed but sort of ordered by the Directors of the Intramural Programs. It was their ideas which dictated where research should go. That was troubling because it was contrary to Dyer's principle. It wasn't just Dyer; there were a lot of others involved in this. So that was the basic conflict and it was never resolved. Just like in any war, there is a victor and there is the defeated and, unfortunately, the Extramural Programs became the defeated. Now, who writes the history? The victors, right?

GC: Right.

PS: And that's how it comes out and in most of the official stuff that comes from the NCI, you read that point of view. For instance, one of the great myths of the Cancer Programs was the myth of the establishment of the Cancer Center Program. It was picked up by some of the new people who had come in when President Nixon's Advisory Board. . . . He went along with it because he was interested in supporting cancer, but people on the committee, such as Farber, the director of the M. D. Anderson Cancer Hospital, Lee Clark, who was a good friend of mine (but not very friendly when it came to this point of view). So, he wanted these Cancer Centers set up by fiat and there should be no real review of them. They would be approved in a separate budget of the Cancer Center. So the myth got started that that's when the Cancer Center Program started. Well, that was nonsense.

The Cancer Center Program was started many years earlier. In fact, when I became the Chief of the Extramural Programs at NCI, the Cancer Center Program was rather dormant and, as a result of some bad decisions taken earlier, money was given to the medical schools. I'm going to talk about Cancer Centers quite a lot. That's really what I really did. The Cancer Centers were established before I became a Director there [at NCI] and the way that they operated that Program was that they were giving money to various medical schools in order to establish a Cancer Center. Well, what was a Cancer Center? Nobody had any real ideas what the Cancer Center was or what it should do. It was just an idea. And so they gave this money to schools like Harvard, Johns Hopkins, Roswell Park, and M. D. Anderson, especially, and they just used the money.

GC: However they wanted?

PS: However they wanted. And in M. D. Anderson, of course, they *did* use it for cancer, but they had their own idea what to do, mostly clinical support. Some of the places just used the money for anything they wanted. It was the Dean's slush fund, the Medical School Dean, of course. It was his slush fund. Well, when that became apparent and they couldn't really evade that issue any more, the Program sort of died and became dormant. When I came over, we looked at that question and said, "Well, what *is* a Cancer Center? Anybody got any idea?" I had a Deputy by the name of Bill Walter. Dr. Bill Walter. He died last year, unfortunately.

GC: I'm sorry.

PS: He would be a good guy to interview. Bill thought about it and we talked about it. I brought in some members of my staff and we talked about it. What is a Cancer Center? We evolved a set of principles for Cancer Centers, and this will be in some supplementary material that I'll give you. It's all in my files down at UTMB. The idea was to make cancer a central program for the University, regardless of the discipline, and to use the approach for therapy of cancer on patients and all disciplines should participate and make the best judgment they could about which modality to use.

That was an awfully difficult concept to get across to people, especially to Medical School Deans, who in many cases were subject to a lot of influence from their department chairmen. We pointed to M. D. Anderson, which was not a good example, because they had misused the previous programs, but said "Well, at least they're devoted to cancer. They treat cancer like it's a disease that needs to be treated with the best possible modality."

Unfortunately, I didn't realize at the time that wasn't true. M. D. Anderson did it just like any other medical school. If a patient approached a surgeon, the surgeon would use his approach. If he approached the medical people, they'd give their approach. And, of course, the other one was radiation, and they would use theirs. So, they rarely got together, seldom talked or discussed the disposition of the case. Oh, they had oncology boards and so forth, but it was the influence of the department chairmen who dictated what modality would be used on a patient. I admit that there was a lot of controversy about it—which modality was better—but still, nothing is hurt by consultation, and this is what they didn't want to do.

So I started going around to many of the leading medical schools in the country, talking to the Dean about this concept. Well, at first, the Deans didn't want any part of that. They didn't want to have a colossus there that was taking one particular disease and putting all their energy into that disease and having surgeons and medical oncologists and radiation therapists all working together in the Cancer Center. They opposed that, most of them. It was a difficult concept to get across.

But gradually, one by one, starting with the University of Alabama—the University of Alabama was the first university that really set to work and established a Cancer Center along the principles that we laid down. Mainly, not completely, but by and large they bought the principle. And others would follow. Harvard University was another one and Duke followed the lead.

That was the beginning of the Cancer Center Program and it started in 1967, not 1971. In fact, by 1971, all the Cancer Centers were pretty well—most of the ones that are supported today—were established, except for these special centers that were established by presidential fiat in 1971, like M. D. Anderson, the Harvard one, which was Sidney Farber's pet. [Laughs] Oh, Sidney was a good man and he worked very hard getting money for cancer and I must say, through his efforts and Mrs. Lasker's, a *lot* of money for cancer was made available. For your information, implementation of that left somewhat to be desired.

GC: Now, were the med school deans opposed just because this was something they had never thought of before? Or what was their opposition to having this interdisciplinary . . .

PS: Because it dissipated control of the departments away from the Dean to a new official called the Director of the Cancer Center.

GC: Okay.

PS: And they didn't like that. Even when the Director of the Cancer Center was subservient to the Medical School Dean, it wasn't the same, because he [the Director] had the money. They wanted the money. If we had said to the Dean of the Medical School, "Okay, you set this up, you take care of the budget for us," there wouldn't have been any opposition. But we didn't want the medical school Deans to get a hold of it because of what had happened ten years previously. There it was. But, in any event, the history as it is written now starts with 1971 and is pretty much according to what you have heard probably. You haven't heard any of this, I'm sure.

GC: No.

PS: And you won't! You won't, from anybody that wasn't in the Extramural Programs and, unfortunately, most of the people in the Extramural Programs are long retired or passed away.

GC: Now, you were saying earlier that the Extramural Contract Programs originally were not reviewed, but then the Intramural people wanted to be involved in the Extramural review.

PS: Yes, yes. They are now, because, as I say, what happened was control of the budgets of the Extramural Program was transferred to the Intramural Program. And, you know, they live according to the Golden Rule.

GC: And you also said that some of the Extramural Programs were actually linked to Intramural Programs?

PS: I say that and I'm really not sure of that, but that's a suspicion. They still maintain the external review. The external review is done by the Division of Research Grants, which is not under any one Institute, and that's still independent, as independent as it can be. Their research is really pretty good. The review system leaves a lot to be desired. They're buried in paper and application approval rate is very low, and when the budget is low . . . you know, a lot of investigators are really fed up with the system, which leaves them hanging in their air if somebody in the Study Section doesn't like the individual person, and that happens.

GC: Really!

PS: I've sat at Study Sections where one member of the Study Section who is very influential damned an application because he didn't particularly like that direction of research. The whole thing is . . . the planning—this idea that everything has to be planned. Charles Kettering of General Motors made the remark—you've probably run across this—that "planning in research is the sure way of going wrong with confidence." And that's true! If you plan, well, this is what we ought to be doing, and somebody comes in with that kind of research, oh, that's right, it's according to plan. Well, you could be wrong. The plan could be wrong.

In the Extramural system, independent university investigators plot their own way and subject it to scientific review by their peers. If that's what really happens now, then that guaranteed pretty much independent approaches, except for those areas which are hidebound by science itself. It's hard to get new ideas introduced.

That's why they've started such things as the new approaches in this, new approaches in that, so that people with different ideas can get some kind of a chance of getting funded, because otherwise the Study Sections are controlled by people who are very influential in some branch of science and who feel *t hat* is really the only way to go, but it might not be. So, if somebody comes in with a kind of a crazy notion and it's a different idea . . . it was like vitamin C, you remember the controversy about vitamin C? Linus Pauling. Linus and I bemoaned this whole situation many times. He couldn't get anything approved, because what he wanted to do was to test vitamin C as an effective clinical agent against cancer. When that came up to the Study Section, it was pooh-poohed away. "We know that vitamin C isn't effective in cancer!" Linus Pauling would say, "Well, *how* do they know? It's never been tested."

[End Side A, Tape 1]

[Begin Side B, Tape 1]

PS: Did you know Linus Pauling?

GC: No.

PS: Have you ever met him?

GC: No. Did you know him?

PS: Oh, yes! He was a frequent visitor to my office, because he wanted support for his research, and the best I could do for him . . . I said, "Well, I can't guarantee that, because this is the Extramural Program. If you can get a contract from the Intramural Program, maybe they can support it." They wouldn't. But the Extramural people had pooh-poohed his research. So he finally, very late on, not too long before he died, got a grant approval and I don't know how his research went. I haven't kept up with it.

GC: I can't believe *he* would ever get turned down for anything!

PS: Oh! Oh, yes. In that sensitive area, yes. They're very picky about that. They used to laugh at his application.

GC: Really?!

PS: In the Council. "This is Linus Pauling," I used to say to myself. "What are you doing?!!"

GC: Wow! So it sounds like you're very against planning research, and I know that was a lot of what was going on during the 1971 Cancer Act.

PS: Yes, that's right.

GC: Can you talk about that?

PS: Well, the National Cancer Act was formulated by this committee which was chaired by, oh, Sidney Farber, and Lee Clark from M. D. Anderson, and other prominent, politically prominent, people. They made sure that their ideas would be the ones that would carry the day. Their idea was to give permanent support to people at Roswell Park, Memorial Institute, Harvard, M. D. Anderson, and the one in Philadelphia whose name I forget, and Sloan-Kettering Memorial. These were the ones that should be supported without review.

GC: Just permanently supported?

PS: Permanently supported. Sidney Farber had that idea and urged the establishment of these political, permanent Cancer Centers, which of course I opposed. They came up with some sort of euphemism for that which they called part of the Cancer Center Program— describing these particular centers as established, recognized Cancer Centers. That was the word they finally came upon. I said, "Well, okay, recognized is okay, as long as it's recognized by peers, not by the NCI." That sort of went for a while, but I don't think it was very important until the day in the Cancer Institute when Sidney Farber, who was a member of the Advisory Board, had his application for his Institute, which had been supported [snaps fingers] just like that, came in with an application that had to be reviewed.

Well, surprisingly enough, even though this had received sort of moderate support from the Cancer Center Review Committee, when it went to the Council, the Council said no. So, they turned it down with the promise of one year of support, but they had to come in with a much better description of what the Cancer Center proposed to do. Sidney Farber almost went through the roof at that decision. That was the beginning. The National G16

Cancer Advisory Board wasn't too bad. They made some very interesting decisions. I'll give you an example.

GC: Okay.

PS: Do you know Baltimore at all?

GC: A little bit.

PS: Well, Johns Hopkins is *the* premier Medical School there, and so naturally, Johns Hopkins wanted a Cancer Center, like everybody else, but they had not been included in this blanket support. So, we went there to talk about this. They wanted to set up a Cancer Center and they wanted to apply for one to the Extramural Program. We described what our purpose was and what a Cancer Center should be—and by that time our definition of a Cancer Center was well laid out in print—and they agreed to do it.

So they came in with this application, which was pretty good—they had some good people at Hopkins—and it was reviewed by the Cancer Center Review Committee. In fact, I was present at the site visit for that and the Cancer Center Review Committee recommended that it be funded. It was rather lukewarm, because there was some question about it, where this center would be located. The Medical School Dean had decided that he was going to locate the Cancer Center at the old Baltimore City Hospital, which was under the control of Hopkins.

Well, that was some distance from Johns Hopkins and far from basic research. One of the requirements for a Cancer Center was that basic research should be integrated with clinical research so that the advances in basic research could be applied clinically as soon as practical. So, that was one of the points that the Committee was kind of lukewarm about. But when they went to the Council with the approval—and in those days there was enough money to approve most any application, as long as it got approved—there was a little problem with the Board, the National Cancer Advisory Board.

The Chairman of the Board was Jonathan Rhoads, a great surgeon who was Chairman of Surgery at the University of Pennsylvania. When that came up for discussion by the Cancer Board, of course, everybody was more or less for it because it was Johns Hopkins. And Jonathan said, "Now," he said, "wait a minute. Hold on here. You're approving a Cancer Center which is about four or five miles from the campus of Johns Hopkins University." It wasn't that far, but that's what he said. "And there is very little if any basic research going on there. How is that going to get involved?"

Of course, the defenders of Johns Hopkins said, "Well, of course, Johns Hopkins will coordinate this, the Dean's office will coordinate it." "Well, what about the Cancer Center Director? Where does he fit into all this?" They all hemmed and hawed. Well, the result was that the decision on the application was deferred until this question of locality was resolved.

So, after the meeting was over, Jonathan came over to me and said, "Palmer," he said, "I'm going to visit the Dean and I would like you to come with me." I said, "All right." (I can't give you the Dean's name; it's long gone from my memory bank.) I said, "All right, and we'll arrange a date." So we set up a date, in a few days, coordinated it with the Dean's office.

So we went in to see the Dean and we talked about this, pretty much indicating the discussion of the Review Board. Jonathan, being a surgeon and very forceful—you had to know Jonathan—said, "Look," he said, "a Cancer Center is set up to coordinate research in cancer, coordinate basic research with clinical research. Tell us how this is going to work with the Cancer Center set up down at City Hospital?" Well! The Dean began to backpedal a little bit. He didn't know. I guess nobody had ever discussed this, except they had some space down at Baltimore City Hospital and that's where they wanted to put the Cancer Center. Jonathan said, "Well, we can't approve this." He said, "You're going to have to find space right here on campus for your Cancer Center." The Dean said, "Well," and he swallowed. This was a big, big, big grant—over a million dollars. And he said, "Well, let me think about that." "Well," Jonathan said, "You'd better think about it, because you won't get this grant unless you do." So, that was the end of that interview.

So, a couple of days later the Dean called me and he said, "Palmer, what about this thing?" And I said, "Well, Dr. Rhoads is Chairman of the Advisory Board and this application will never be approved by the Board unless you do what he says. You'd better find some space on campus, like I told you in the beginning." So, he said, "Fine." He called me, a couple weeks later he called me and said, "All right, we've been able to make some space available for the Cancer Center right here on Wolfe Street." And I said, "Well, that sounds good. I think Jonathan would approve it." So I called Rhoads and told him that and he said, "Yes, that will be fine." So, that was how it was approved.

Imagine my pleasure when I came back to Baltimore just a few months ago, drove down Orleans Street, and saw this marvelous new edifice being constructed, "The Johns Hopkins Cancer Center" right there on the campus.

GC: Right on campus.

PS: Right on campus. A great big one. So, they have gone out of their temporary quarters, which they eked out of some other program that had been falling away. Now they've got this great big building. It's quite a place to see.

GC: Really!

PS: So, I'm kind of proud of that.

GC: Because you were right in the first place.

PS: Well, I think I was. Time will tell.

GC: Jonathan Rhoads sounds like quite a character.

PS: Oh! He was! He could tie a knot. But, oh, he and I got along very well. In fact, when I left for Texas, the whole Board gave me a dinner. Had a wonderful time. Jonathan was really up to form then. Told a lot of amusing stories.

GC: About you?

PS: Not so much about me. Just amusing stories. Some I can't repeat.

GC: So you had a lot of interaction with the Board.

PS: Oh, yes. And with the Study Section members. I found my way around most universities around the country and got to know most of the leading people in science that way.

GC: Oh, really.

PS: It was really a wonderful opportunity. I talked about this problem with the Medical School Deans . . . when we went up to Tufts University in Boston to talk to them about a Cancer Center, we talked to the Dean quite a while about this concept. And he said, "Well, that makes sense. That's reasonable, to coordinate all this for the benefit of the cancer patient." I said, "Yes, that's the idea." And so he said, "*But*, you're going to have to talk to all the Department Chairmen to see, you know, how they're going to feel about this." So we talked to Radiation Therapy and they were all for it. And we talked to Medicine. While they were reluctant, they decided that Medical Oncology ought to join the Center. And then we went and talked to the chairman of surgery. I'll never forget what he said. We talked about this and told him exactly what we had in mind, and he said, "Now, you know, we'd like to cooperate with you. Actually, the Director can see any of our patients at any time." *Our* patients! That's what they didn't want to lose, *their* patients. That gave me the clue as to what really was happening here. They didn't want to lose control of the budget, patients, so forth. No. Well, so there we are. That's about the size of it, I think. I have a lot of actual stuff that I can send you. They're in my files, papers giving details and dates and names and so forth. Talking about the old Testing Program, the Clinical Programs, which have been subject to a *lot* of criticism because of the way they were funded and so forth, but they did some good. The clinical leukemia group and people like that. The program at the hospital in Memphis, Tennessee, Danny Thomas's was part of that program.

GC: St. Jude's.

PS: Yes. And they have done a *lot* of good. Because of their program and the fact that they are working in that particular area of leukemia and have come up with some very good therapies, children have a much better chance now of surviving some of the leukemias than they did before, when it was almost certainly a sentence of death. You know, we have a few victories like that. By and large, of course, cancer is almost intractable, no matter. All the breakthroughs that we try, all the chemotherapeutic agents, radiation therapy, which is tried, is getting to be pretty much of a disappointment.

Another program that I had a lot to do with in the Extramural Programs at NCI was the Radiation Therapy Program. I got to be quite active in that and promoting the idea of using high energy particles and getting them established with neutrons and cyclotrons and so forth in various places. Even got one installed in M. D. Anderson. That was quite a victory, because they wanted to use their own machines, but we talked them into getting a cyclotron. Apparently that's been very successful.

And a lot of these new developments, like the gamma knife, which is . . . of course with the development of computer tomography, very good. Now they can mark the area of the tumor in the brain and fix the head with clamps and so forth—you've seen those things—and you can, with a good computer program, focus the high energy beam on the tumor and in many cases it is successful. It didn't work with my son. My son died three years ago with a brain tumor.

GC: Oh.

PS: But that was his last, the therapy of last resort was the gamma knife, and we did that but it didn't work. It relieved his symptoms for a while, but he went downhill and he died.

GC: I'm so sorry.

PS: Right in the house here. He was in our home. Unfortunately, his wife didn't have much tolerance for what was happening to him and he came down to Galveston to see if I could help him. The only thing I could do was to try to get a neurosurgeon to treat him. It was his idea to use the gamma knife. I wish he had had a more accurate computer program, but this was as good as he could get. But anyway, my son had a stage II, probably early III, oligodendroglioma, which is close to an astrocytoma. Anyway. We have our failures, more failures than successes, I'm afraid. But that's the story of science, isn't it? We make progress through our failures.

GC: How did you come into cancer research? Was that at all expected that you were going to end up at the Cancer Institute or in cancer research when you started doing pharmacology and toxicology?

PS: No. I was searching for new pesticides, trying to find a good rodenticide. We got some successes. We were also trying to find a systemic agent that would kill insects on the body. That wasn't very successful. And, of course, being a pharmacologist . . . when I first went over to NIH, I became the Executive Secretary for the pharmacology study section.

GC: When you said—that was something you were going to go back to—you said that when you were at Galveston there was a possibility that you might come back here and that was a story you wanted to go back to. What was that?

PS: Yes. That was to come back to NCI and work at NCI. Benno Schmidt, have you heard of Benno Schmidt?

GC: I've met him, actually.

PS: Yes. How is he? He's had a cerebral problem.

GC: He's doing all right. He still goes in to the office. He's doing all right. He's very healthy.

PS: I see. How's his son? His son has the same name, Benno Schmidt.

GC: Yes. I didn't meet his son.

PS: Ahh.

GC: I went to his [father's] office.

PS: At Yale, I think. He [Schmidt, Jr.] was president of Yale, wasn't he?

GC: He *was* president of Yale. He's heading an educational foundation now, I think, but I don't know exactly what he is doing.

PS: Well, Benno and I were pretty close. He was one that really put a lot of pressure on me not to leave NCI. He didn't want me to leave, but I turned him down, and I guess he didn't like that.

GC: Was that part of what made the decision to leave so hard? To go to Texas?

PS: Yes. Yes. When someone like Benno Schmidt calls you up at home and asks you not to leave, to stay.

GC: How did you work with Dr. Zubrod? He's one of the people you said that . . .

PS: Oh, Dr. Zubrod was the head of the Chemotherapy Program, when it was a Program, and he was also a highly regarded clinical investigator. I was a member of his staff. He was also chairman of the leukemia groups, A, B, or C. I forget which one.

GC: I think it was B.

PS: B. And he did a lot of good work in his area. When they made him head of the Chemotherapy Program, he continued that but, of course, expanded his interests into other areas, and wanted me as his deputy. One of the projects I started there was getting support for the screening of plant products for possible anticancer effects and we funded a large project at the University of Wisconsin to make a catalog of cancer-active plant products. We did a lot of that. Some of the contracts that we had for screening cancer drugs were under our control. And I actually had a review function, too.

GC: In his program?

PS: In his clinical trial studies and we had several. I persuaded Gordon that we should set up a committee of outside experts to review some of this, and he agreed, so we did. I think that was a good step forward. Of course, then I was pulled off of that to do the Richardson Committee report. Have you heard of the Richardson Committee Report?

GC: No.

PS: You probably haven't. Probably haven't even seen it.

GC: No.

PS: Of course, after I presented that to Dr. Shannon, it was put on the shelf. He didn't like the report, so it never saw the light of day.

GC: And this is the one that said the Chemotherapy Program was doing a good job?

PS: A fairly good job. I know there were a lot of criticisms. It wasn't a whitewash by any means . . . they called a spade a spade.

GC: And you didn't know exactly why Shannon didn't like the Chemotherapy Program?

PS: Personally, Shannon, you probably never knew Shannon?

GC: I didn't. He died a couple of years ago. Is that right?

PS: Yes.

GC: So, when you were at the NCI, did you do any, you didn't have your own laboratory. Were you doing research or were you purely administrative?

PS: Purely administrative. I set up a new category of scientist-administrator, and I was on the Civil Service Board for scientist-administrators, a classification they had never had before.

GC: Oh! So, you helped create that category.

PS: Yes.

GC: Was that hard to leave research to do administration?

PS: Not really. I had run out of steam in the laboratory. I was getting to the point where, well, my wife and I had two kids and I had a lot of things at home and so on distracting me. My own fault. I didn't really devote myself to the laboratory research, that I should have been doing.

GC: So it was fine to make that move into administration.

PS: Yes. I felt a sort of relief to get out of the laboratory. You know there are a lot of people in the laboratories that never become Nobel Laureates. You've got to recognize there are two kinds of research. There's big research and there's ordinary research, and the big research features the people who get Nobel prizes. But they have to be assisted by a lot of people who just do the ordinary support work but move things forward; not everybody gets credit. I've always been one to try to give everybody credit who's working in the program. A lot of people don't do that. So, while leaving the laboratory was difficult, leaving laboratory research wasn't especially.

Well, I tell you, there was a sort of epiphany there. All of a sudden I felt very reluctant about taking live animals and performing research on them, which you have to do in pharmacology. You know, you walk into the laboratory every day and see all these dogs stretched out on boards, with all kinds of tubes and so forth in them, and you know they'll be killed, and I got very upset about that. But the main thing was the cats. I love cats. Always have. [Tape background noise]. I said, "Oh, God, how can you *do* that to a cat?!" "Well, a cat's no better than a dog or a rat. You do it to a rat. You kill him, Palmer." Well, that was true, because toxicology in those days was based mainly on death. Kill 'em and count 'em. That was the basis of toxicology. How do you know whether a drug was toxic or not? You inject it into a mouse or a rat, and you do so many things. You measure such things as the LD 50, a dose that kills 50 percent of the experimental animals. So, toxicity of compounds is measured in terms of this LD 50.

Well, the whole concept of having to kill little things became to me distasteful, and I think that probably drove me out of the laboratory more than just that I didn't want to do the research, the laboratory research, or that I was too lazy to do the laboratory research. It was distasteful to know that what I would have to do, to come in to the laboratory and take some poor helpless animal and sacrifice it. I'm not an animal activist. I don't go around burning laboratories. I think that's disgraceful. But I just say I personally don't like to inflict that. I would hate to kill a rabbit out here or a chipmunk. I even get to the point where I hate to kill cockroaches! But, you know, I guess I carried it to an extreme, but that's the way I feel.

GC: And that's something you couldn't avoid if you stayed in pharmacology?

PS: Yes. Staying in pharmacology research. What I did enjoy in the laboratory and never had a chance at, began when we were looking for new rodenticide compounds. I began to synthesize compounds in the lab. Going back to my old organic chemistry days. Oh, and I began to *really* love that. I could visualize chemicals running through the reactions and calculating the yields and so forth. I liked that. So, I think chemical research was very good. I began to think about how can we measure toxicity in terms of chemical terms.

When I came down to Galveston, I was a professor of pharmacology and also a professor of preventive medicine and community health and I had a laboratory over there. One of the things I did was to look into alternatives to live animal research. I used brine shrimp once as a measure of toxicity.

GC: Brine shrimp?

PS: Brine shrimp, yes.

GC: Oh, I saw that in your bibliography.

PS: Yes. So, that was, I thought that was a good way to go.

GC: And it worked pretty well?

PS: It worked pretty well, yes. There's a place over at Hopkins that has alternative research activity. I forget the name of the man that's doing that. They've had a whole committee set up on that. They have foundations that have given him money because, animal rights foundations have given him money to find ways of doing things without using live animals, or live mammals. They use insects. Apparently it's pretty legitimate to kill insects, but you can't do it to mammals.

GC: They're not as cuddly, I guess.

PS: I'm not sure of the ethics of that. It escapes me. But that's what they say. Even brine shrimp are living!

GC: So, were you involved at all in Dr. Zubrod and Dr. Frei and Dr. Freireich on that big leukemia . . .

PS: No. You mean the clinical?

GC: Right.

PS: No. I was not involved in the Clinical Programs at all.

GC: So you weren't reviewing them or anything like that?

PS: No. I was in the testing of chemotherapeutic drugs.

GC: Okay. So, what is your feeling on contracts being used for research? I know there's been kind of an ongoing controversy . . .

PS: Oh, yes.

GC: And discussion of . . .

PS: Still is.

GC: Contracts versus grants versus different kinds of funding mechanisms.

PS: Yes. Well, I've always been opposed to it.

GC: Opposed to contracts?

PS: Opposed to contract research, *except* I came to moderate my view somewhat when I realized that in order to develop things like new substances and test those new substances, there was no way in the world that a Study Section composed of top scientists in the country would want to devote any money to that thing. So, maybe they did approve these things, but with a priority so low that it couldn't be funded. So that, I thought, well, that may be an area where contracts are justified.

So in the Chemotherapy Program, for instance, those contracts were used for that. They were given to drug companies to test their own drugs, which I think probably was not a good idea, but anyway it was a way of getting things going. Because the drug companies didn't want to do that on their own. They couldn't see any real earning capacity for any drugs, so it was very difficult to get the pharmaceutical companies to do the testing.

Of course, now it's different. They've got a whole new approach to things with the new biology. Whole new vistas have been opened in terms of chemotherapy and, you know, studying biological reactions in cell biology. Cell biology is opening new avenues and there have been a lot of developments in that area. So, that's a whole new ball game, as far as I'm concerned.

GC: Have the pharmaceutical companies always been involved with the NCI in some way?

PS: Yes. They've always been involved.

GC: Really?

PS: Yes.

GC: Has that relationship changed over the years?

PS: Up to the time that I had anything to do with it, no, it hadn't changed. Whether it has changed now, I don't know. I'm not in on that any more. I'm off, quite removed from that. You'd have to talk to somebody who's in it now and has been in it for a long time to see whether there has been any change in the relationship between NCI and the drug companies. Of course, there are groups, and fanatics that say NCI is in cahoots with the drug companies. You hear all that stuff, but I don't know enough about it to discuss it any more. I probably would have had a view twenty-five years ago, but not now.

GC: When you were in the thick of it?

PS: Yes.

GC: Well, twenty-five years ago, or a little bit more, when the National Cancer Act was being worked on . . .

PS: In 1971, yes.

GC: In 1971, what was your involvement in all that? Or did you have an involvement, or was it just happening . . .

PS: None whatsoever. We were not involved at all.

GC: Anybody in the Institute?

PS: Well, the NCI people were involved, but not the Extramural Program. We were excluded from it. We never got a look at it.

GC: Really!

PS: Of course, that went against the grain quite a bit. That was true of the Heart Institute, too. The Heart Institute had an association with the American Heart Association.

GC: So the Extramural Program was totally excluded from anything . . .

PS: At that time, yes. In the Cancer Act era. Yes. We weren't considered worthy of notice about that.

GC: But did it affect what happened in your programs?

PS: Ah, yes, it did, absolutely, because of the Cancer Program, because we had the existing Cancer Program which now they say didn't exist before 1971. But the fact is, when they created these special [cancer] centers, the responsibility for that was given to me.

GC: Were you also aware of all the planning that Dr. Baker was doing as Director about planning research and where research should go and all that?

PS: Oh, yes. I sat in on a lot of those.

GC: Did you go out to Airlie House for those meetings and everything?

PS: Yes. Yes, I did.

GC: What did you think about that or what was your input?

PS: Well, I subscribed to the Kettering view: Planning in research is an excellent way of going wrong with confidence. I kept repeating that. I said, "You can *not* plan research." "Oh, of course you can!" "No you can't." So, I was intractable on that one. Nobody could change my mind on that.

GC: Did those plans end up affecting the way the Extramural Program ran?

PS: No. Not at the time I left, anyway.

GC: You were still working there when Dr. Rauscher came in. Is that right?

PS: Dick Rauscher? Yes.

GC: And how was he to work for, or what do you remember about him?

PS: Well, he's gone now. God bless him.

[End Side B, Tape 1]

[Begin Side A, Tape 2]

GC: Continue with Dr. Saunders, July 22, 1998. Okay, we had just started talking. I had wanted to ask you about some of the Directors you worked under, and we had just started talking about Dr. Rauscher.

PS: Dr. Rauscher, yes. Dr. Rauscher was in the Intramural Program, so therefore he's an Intramural person. And he was a very clever young man. He was a biologist and he discovered a virus that was named after him in his honor. He was a good Director. A very good friend of mine and very pleasant. Worked easily and readily with most everybody. I don't know how to say this, but he was so agreeable to, whatever you wanted, he would agree. Whatever opinion you had, he would agree with you. But then he'd make up his own mind as to what it should be. So, that was his real quality, I guess. He was very discriminating.

He was another one that didn't want me to leave NCI. He really, really did make an effort to keep me there and I was surprised to learn after I left, he left and went to the American Cancer Society.

GC: Right.

PS: And after the American Cancer Society, he moved on to a drug company, I think it was. And, unfortunately, poor fellow had a heart attack. His wife is named Peggy.

GC: Peggy?

PS: Yes.

GC: You knew her too?

PS: Yes. I don't think she knew me very well, but I knew her. He always used to talk about Peggy. He and Peggy and Marge, my first wife's name, Marge and I used to go out together once in a while. When Danny Thomas was on the National Advisory Cancer Council, he was very interested in our program. It's too bad he died. I'm sure he could tell you a lot about our program. He got to know us pretty well. Danny Thomas, as you know, was very active in the Arab-American, the Lebanese-American, Charities, and they, of course, were very instrumental in funding the hospital in Memphis, the leukemia hospital, children's hospital, St. Jude's. Jude, of course, is the patron saint of hopeless causes.

GC: The patron saint.

PS: The patron saint of hopeless causes. I asked Danny one day how it ever happened that he did that. So he said to me—I was at breakfast with him and his wife—and he said, "Well," he said, "when I was in Chicago, I was in vaudeville and was making a pretty sorry mess of it and I didn't know what to do. I was poor. I went to a church" [he was a Catholic] "and prayed to St. Jude, who was the patron saint of hopeless causes, and somehow got the message that I should do something for charity." And he said the very next day he was offered a job that led to his big . . .

GC: His big break?

PS: Big break. And so he never forgot that his dedication to St. Jude and what his message was from St. Jude to do; he'd made a vow. That's why he did it. So, he went down to Memphis to this institute there which was struggling along on a few research grants from us and went to them and said, "I would like to fund an institute here." That became the St. Jude's Hospital. That's how it all happened.

GC: Wow! I never heard the story behind the . . .

PS: Yes. Probably very few people in the world ever knew it. That's how Danny Thomas became the patron saint of St. Jude's Hospital in Memphis, Tennessee. He invited Marge and I and Peggy and Dick Rauscher to their big party. They had an annual meeting in Washington and they had a big party at one of the hotels there and so he invited us as his guests there. That was a lot of fun. He and his wife, so there were three couples. (One of the couples was Dr. Bill Walter and his wife. Dr. Walter was my deputy.) He said, "I have a surprise for you, Palmer." I said, "What is it, Danny?" He said, "Guess who's coming to the hotel tonight." "I don't know." He said, "Well, my daughter, Marlo." I said, "Oh, wow, Marlo Thomas!" He said, "Yes. She's in New York but she's coming in and she's coming right down to be with us." I said, "That will be a lot of fun, Danny."

So, you know, we were carrying on and finally he kept checking his watch. It gets later and later, and no Marlo. So, he goes outside and he says, "I'll be right back." Apparently he had gone to the desk and asked if his daughter had come in. "Oh, yes. She came in about an hour and a half ago." He goes up to the room. She's in bed, asleep. She apparently said, "No, Dad, I don't want to go. I'm too tired." Well, poor Danny. He was crestfallen. He came down, gave us the bad news that Marlo wouldn't be joining us. He was so upset. He was really angry at her!

But Dick Rauscher was there and it was good fun and I really enjoyed Dick. When I left, I don't know that he left because I did, but he did anyway. Why he left, I don't know, but they made him an offer he couldn't refuse, I suppose. So, that was Dick Rauscher. As an administrator he was good. I can't say anything about him but good.

GC: And so before that was Carl Baker, right?

PS: Yes. And that's an interesting story, too. Carl, you've met Carl? You've talked to Carl? Probably told you all he wants you to know about him, I guess. Carl became Director right after Ken Endicott moved over to the National Institutes' office on the campus. And I don't know whether he had been the Planning Officer. He believed in planning. He was always having planning meetings. He had a lieutenant, Lou Carrese, who used to run these planning seminars.

GC: Lou Carrese.

PS: Lou Carrese, yes. He has since passed on. Lou Carrese was instrumental in organizing these planning sessions and Carl thought that was great. He and I used to argue. When he was director he learned about the nature of extramural research and, while he didn't buy it all, he was at least sympathetic and used to kid us about it. But he was really good, very, very good. I enjoyed Carl very much.

But when the Cancer Act was passed in 1971, Carl apparently ran afoul of some of the big-wigs in the cancer [research] field. The *Science*, the journal *Sci ence*, had a writer, whose name I forget. She was a woman who wrote a scathing article about Carl putting on as a military officer and he was going to be the general in charge of the new "War on Cancer" Program. Real critical article. Perhaps, as a result of that, Carl was replaced by Dick Rauscher. One of the influential members of the Board, a presidential appointment, yes—I forget his name, he was a big official in a drug company—called him down in Florida and took him out on his boat to talk to him about taking over the Cancer Institute and what he wanted. So, Carl was out. Carl was a good man.

GC: So, Dr. Endicott was before that, right?

PS: Yes.

GC: You said he used phrases, like he said someone wanted to be a "belly doctor." It sounds like he was quite a character.

PS: He was. Ken Endicott was Director for a long time after Rod Heller left. Rod Heller was the Director and went to Sloan-Kettering and unfortunately had a stroke and became quite incapacitated. Later he came back to NCI and did some part-time work, but he never really did much after that.

Ken Endicott followed Rod, and Ken, as I say, was Director. He was the one who invited me to become the Associate Director for Extramural Programs . . . although he was really an Intramural person, he comprehended Extramural research and certainly worked hard for us. But again, he was an outspoken person and ran afoul of Sidney Farber and Mary Lasker and he did it in a very innocent way. He said—I think it was a slip of the tongue, although knowing Ken it may not have been—he said, "Mary Farber and Sidney Lasker

. . ." Well, that was the end of Ken Endicott as Director.

GC: Really!? They didn't like that, huh?

PS: Didn't like that. Went to Shannon and said [whispering] "He's got to go." So he moved over and got some administrative job over at NIH. Ken Endicott was a very active Director and very supportive of the Research Program and very supportive of the Center Program. He was very happy with the way the Center Program was being developed and never hesitated to tell me so, which I appreciated very much. You know, people always tell when you're doing the wrong thing but they won't tell you when you're doing the right thing, but he did. He was very happy with the way things worked out. So, I was sorry to see him go, because he was a good man. Very knowledgeable.

I'll tell you an amusing story about Endicott. He was Director when the first Surgeon General's report came out about smoking and there was a big to-do in the newspapers about it. He and I were in Chicago and I think Dick Rauscher was with us, too—several others who were on the staff—and we were holding a big meeting there for some reason or other. He was always holding a meeting. Ken loved to hold meetings. Ken was a chain smoker. He says, "Fellows, I've got a problem. I know *very* well that they're having a news conference tomorrow and I know what it's going to be on. It's going to be on the Surgeon General's report. And I know what they're going to say to me. They'll say, "Do you smoke, Doctor? So what can I say? I do." So we looked at him, and said "Why don't you stop smoking, Ken? Just give it up. And then when they ask you, say 'No, I don't smoke.'" In all honesty.

So the next morning at the news conference we were all standing around. You know how those things are. "Doctor! Dr. Endicott!" from a reporter. "Do you smoke?" "No. Don't smoke." And then he follows up with "Did you ever smoke?" He says, "Oh, yes. I smoked." "Well, when did you stop smoking?" He hesitated and said, "Last night." That brought the house down. Do you know that that never appeared in the interview?

GC: Really?

PS: Yes.

GC: Did he start smoking again, or did he . . .

PS: Oh, yes.

GC: Did people, when that report came out, did people believe that smoking was a link?

PS: Not at that time. That was the first report. It was after Jesse Steinfeld's report that people began to take notice and later on when Dr. Koop was the Surgeon General, came out at that time with much more positive information because they had more data then. That's when they really . . . who was the Surgeon General then?

GC: [C.] Everett Koop.

PS: Koop, yes. Koop. Of course, he loved publicity and put the anti-smoking program on the map. What else can I tell you about Ken?

GC: So you knew Heller before?

PS: Yes, Rod Heller.

GC: You were not at the NCI though. You were at the NIH.

PS: No. I was in the Division of Research Grants, which was the review body of NIH.

GC: So you still had dealings with Dr. Heller?

PS: Yes.

GC: What was he like?

PS: Oh, Rod, you wouldn't want to meet a nicer man than Rod Heller. He was really something. Yes, he really was. I only knew him in connection with the review process and he didn't interfere with that or anything like that. In those days we were all housed in a set of offices dating from the war, temporary buildings like they used to have in the Navy. I don't know if you remember those, it was building T-6.

GC: Those Quonset huts?

PS: Not exactly Quonset huts but certainly barracks-like structures with several wings. So we had a little blind stand there, you know, run by a blind man who sold snacks and so forth, and we used to always run into the Directors, Rod Heller, and Ralph Meador, who was the predecessor of [Philip] Waalkes, who was my predecessor, was Associate Director for Extramural Programs, and others like that, mostly NCI people. Of course, the others had already moved to new places and they would stop and chat with you. Never passed you by. "Oh, Dr. Saunders." He always called me Dr. Saunders.

"Hello, Dr. Saunders. How are you today?" "Fine. Thank you, Dr. Heller." And he said, "Oh, call me Rod." And I said, "Well, call me Palmer." Then, of course, the NCI moved into the new wing of the administration building where they are now.

GC: Yes.

PS: Well, that's about as much as I can tell you about Rod. He was an old Public Health Service Officer and, like most of them, came out of the Venereal Disease Program, surprisingly. I think Rod did and I think, I know Ken Endicott did, and many of the people who moved to NIH were formerly in the Venereal Disease program.

GC: That's kind of interesting. I wonder why . . .

PS: Because they shut down the Public Health Service offices. My deputy, Bill Walter, was also from the Venereal Disease Program and he was assigned to Florida to do sociological work. In Venereal Disease they always tried to follow up when they would run into a case—especially women—tried to track down what her contacts were and so forth. I guess that was mostly their job, epidemiological work.

GC: You were never a Public Health Service Officer, were you?

PS: No.

GC: You're not commissioned?

PS: No. I was Civil Service.

GC: But you said you were a nutritionist for a while.

PS: Yes. I was assigned to the Public Health Service Nutrition Branch with Dr. Sandy Sandstead; he was downtown in the Division of Chronic Diseases. The Division of Chronic Diseases used to be downtown, and I was assigned to Dr. Sandstead's unit. He had several units around the country to study the nutrition of children and measure their nutritional status. I was assigned to the Washington-area team. We worked out in Baltimore and our job was to go around to various children's homes, any population place like that, and test children for their nutritional status for which we had a nutritionist, medical evaluation for which we had a medical officer, and a biochemical evaluation, which is what I did. So we did those and we wrote reports on them. I think you will see a citation in my bibliography about one of the reports that we have on that. They had a team up in New England, one in the Middle West, and one in the South. So I was assigned to them.

There was a time when Dr. Sandstead said, "Why is that you don't have a Ph.D.?" I said, "Well," I said, "I just haven't had a chance yet. I got out of the Army a couple years ago and I thought I'd go to graduate school." And he said, "Well, why don't you go and get your Ph.D.?" So I said, "Okay." I went down there and I said, "Well, I couldn't do that as a weekend or evening student. I would have to be a full-time student." So I told Sandstead. He said, "Well, what do you mean, have to be a full-time student? Really?" I said, "Well." He said, "Look, why don't you sign up for the program? You'll see. Maybe some of the work you can do in evenings and on the weekends, and some of that even down in the laboratory." I was attached to the Maryland Department of Health.

So I signed up, got accepted to the Graduate School. Went into the biochemistry program under Dr. Schmidt, Chairman of the Biochemistry Department at the University of Maryland Medical School. That's how I did it. It turned out that I had about maybe 75 percent of my time on Public Health Service work, which could be done in the evenings or on weekends. They put you on what they called WAE status, which means "when actually employed." That way I was really able to get through medical school—I had two children at that time—and graduate school. In those days, to take a Ph.D. you had to do the first two years of medicine. So, that's what I did.

I picked my Ph.D. thesis, which had to do with sodium tetrathionate and its action on the liver. That was my thesis. So it worked out very well. You know, while we weren't having full income, it was practically that, and we made it up. By the time I got through all my didactic work, all I had to do was my research. Then I worked out an arrangement whereby I could go back to Edgewood and do my laboratory work at Edgewood. I picked this problem of sodium tetrathionate because it was of interest to Edgewood. It was almost a toxicological program project.

So, that's how I worked it. I worked all my full time there, not only for the Army, but I was able to apply it as a research project. So that's how I got my Ph. D. I was going to say it's good in a way, but in a way I certainly regret not going on a post-doctoral.

GC: Yes. You just went straight back to work?

PS: Yes, because I was already working there. It was easy, but probably not the thing to do. If I had to do it over again, I wouldn't do it.

GC: Would you go back full time?

PS: No. I'd go, I'd pick a year, get an appointment as a post-doc somewhere. I could have gone up to Rochester, and gotten into their program, because one of the professors happened to be interested in my tetrathionate project, so I would have been a natural. But, of course, it would have meant giving up my Civil Service position and, having two kids and my wife, I had to think of supporting them.

GC: Right, right.

PS: So, that's the way it happened.

GC: So when you came to the NCI, who did you work with on a daily basis? I noticed that your job title changed a lot and I wasn't sure if that was reorganizations or actually changing your position or . . .

PS: Some of it was reorganization, some of it was just changing my title. For instance, I became, let me just review what you want to ask me.

GC: Okay. So, you came in to NIH as Executive Secretary, Pharmacy Study Section . . .

PS: Pharmacology.

GC: Sorry.

PS: Please don't make that mistake.

GC: Pharmacology. Sorry. I had it abbreviated. Pharmacology Study Section, Division of Research Grants, and then you went on to become Assistant Chief of the Research Review Grants Branch. So, that was an actual shift in duties.

PS: Yes. That was when I became a Review Officer.

GC: So, in those positions at NIH, were you working with people from all the different Institutes, reviewing . . .

PS: Reviewing all applications. My job was to assign not only the Study Section but the correct Institute.

GC: Okay.

PS: Had to make two decisions. And we did that. We had a staff of three review officers. That's how, incidentally we had some problems there with upgrading those positions in the Civil Service. That's why we dreamed up the scientist-administrator title so we'd get one additional GS rating on that.

GC: Oh, okay.

PS: That's how that came about and so that's how I wound up on the Review Board of the Civil Service Commission.

GC: As scientist-administrator.

PS: Yes.

GC: Is that used pretty widely now, now that that's been established?

PS: I think so, unless they've got some other title for it. But as far as I know, it's still used.

GC: Okay. So, and then that was at NIAID, right?

PS: No. NIAID was something different. That was before that.

GC: Okay.

PS: That was at the time I was still an Executive Secretary. I had done a little bit of application review, but a friend of mine was Associate Director of NIAID, a fellow worker at the Edgewood Arsenal, named Leonard Karel. He was Associate Director of the Institutes of Allergy and Infectious Diseases, NIAID. He asked me if I would like to come on as his deputy. I thought I'd like to try it.

Well, that got to be very political. Poor Leonard got involved with Dr. Nodell, who was an assistant to Jim Shannon, who had come to NIH from the Walter Reed Army Hospital, one of the laboratories there. He was an allergist and immunologist and he wasn't a very pleasant man. Leonard had a bad time with him and got sick. So I had to assume his post and I had to work with this guy, Nodell, and it wasn't very pleasant. He used to shout and was, in my opinion, an obnoxious person. So I decided after a year, that was it. I told Leonard I was leaving, and he said, "Well, I don't blame you." I went back to DRG and I guess I took over another Study Section. That's where I ran into Bert Cole. Have you heard of Bert Cole?

GC: No.

PS: I don't know if he's still alive. The last time I heard he had retired to Florida. He was heavily involved in the Extramural Program. In fact, he was there when I first came there. We became good friends. He was, he headed . . . one of the branches there was the Grants Referral Branch. He headed one of the branches of the Division of Research Grants and we all worked for him in assigning applications. And the next move after that was . . .

GC: Cancer Chemotherapy Study Section.

PS: I took over the Cancer Chemotherapy Study Section, which was the first time I learned much about cancer. That was chaired by Leon Schmidt from the Christ Research Institute in Cincinnati. He was the chairman of that committee. I never really got along with Leon.

GC: That was the first time you learned about cancer?

PS: Yes, yes. The very first time.

GC: That must have been interesting.

PS: It was. Yes, it opened a new world to me. I started to read up on it and I realized what a terrible disease it was—a threat to everybody. And there really was nothing much as far as treatment is concerned outside of surgery. The first step was to debulk. They used to call this "debulk the tumor."

GC: Debulk the tumor.

PS: Debulk, which means going in surgically, remove as much of the tumor as possible. Then to follow up with chemotherapy—and they didn't have much in those days—that's when you'd use chemotherapy. Radiation wasn't thought much of as a treatment, although there were some experiments going on. That's why the Chemotherapy Study Section was formed. There's a long story behind that.

As a result of the formation of the Chemotherapy Program, which was an Intramural program, a contract program, they had formed this Review Committee and the Review Committee had been assigned the job of reviewing all applications for chemotherapy applications. There was a whole lot of problems with that, because that violated one of the principles of the formulation of the Extramural Programs—that the Extramural Programs should be divorced from the Intramural Program. So, as a result of that, there was a big hub-bub raised. It had nothing to do with me, but that was the background.

As a result of that, they decided to transfer the strictly review functions to a new study section which was to be named the Cancer Chemotherapy Study Section. They reviewed, by and large, two types of applications for chemotherapy. They reviewed the strictly screening programs, where they were doing laboratory experiments and trying to screen drugs, finding new drugs against cancers, using mice as a model. That's one group. The other group was the application of research methods to the clinical treatment of cancer, using epidemiological techniques—following up actual experimental protocols they had. There were several groups formed. There was the leukemia A group, the leukemia B group, the leukemia C group. There was the breast cancer, the breast group, and there were other groups like that. To take particular organ sites. That wasn't the origin of the organ site program. That came much later. But that's, essentially that was the beginnings of it.

So, the problem with that was—and as I understood, this was the problem—the Executive Secretary whom I succeeded, Fred Appel, couldn't buy that. He would usually object to any elements that we can. "This is wrong. We cannot be reviewing these protocols." What used to happen is that the people in the Chemotherapy Program of the NCI, led by Stu Sessoms, I guess it was. Do you know Stu Sessoms?

GC: No, but I've heard that name.

PS: If he's still alive, you need to talk to him.

GC: Okay.

PS: Stuart Sessoms. And he said, "We can't buy that." So finally it was decided that the Cancer Chemotherapy Study Section would no longer . . . let me back up a little bit. What used to happen was people in the Chemotherapy Program used to recruit clinical teams to evaluate the drugs that were expected to come out of the pipeline from the chemotherapy studies in the laboratory. They set up grants to find new chemotherapeutic agents which expected to start pouring out and they needed some way of setting up studies for these.

Well, few people had any experience of this in the United States—or anywhere else, for that matter. So, they decided to set up learning teams where they would try existing compounds, I mean. There was, oh, let's see, nitrogen mustard, and that had been discovered accidentally, and there were other compounds that were in use and were standard, so they knew what the results of those would be. They would take those and then set up blind studies where some patients would be assigned the traditional study and other patients would be assigned to the new drugs. But in the absence of those miracle drugs, they decided to compare A, B, and C, and that sort of thing, very closely related compounds and see if they could detect any difference.

So they'd recruit these clinical teams, and they'd tell them, "This is the protocol that you're to study." They would finally round up enough people to do that, talk to a Chairman, like Zubrod, who would agree to do it and they'd get started with the money that the Chemotherapy Program provided for them.

So, when they separated the Program out of the NCI review into the Division of Research Grants review, the application would come in to essentially NCI-sponsored protocols. Of course, the Study Section looked at these, "Terrible! This is not the way to do it!" So they disapproved. Well! That's all you had to hear. The people who ran the program who were doing this solicitation of these groups would come thundering down, hammer on Fred Allen's door and say, "You *can't* disapprove these! These we've already approved and we've already given them some startup funds!" Well, after that the study section . . . wouldn't buy this at all.

They decided then that all clinical, if the NCI was going to fund them through the CCNSC, that was the name of the thing, then they didn't want to review them. If they had already been pre-approved, it was ridiculous to have them come to a study section for approval, so they broke that off. So NCI formed *an other* committee of the Intramural, but not of intramural scientists. They would recruit people from the outside to do these reviews for the program. And Ravdin, from the University of Pennsylvania, was the chairman.

GC: Is that Ike Ravdin?

PS: Yes.

GC: Okay.

PS: He has since died.

GC: I've just heard the name before.

PS: Oh, yes, Ravdin. People knew about Ravdin. He was quite a figure in the early days. And they did that. It was a rather, well, it wasn't very satisfactory. So finally, much later, when I went with Zubrod as his deputy (this was much later), that was when we decided to take over that program, set up our own review committee, and do it that way.

GC: So, were the study sections, was that a full-time position for someone who was called to be in the Study Section, or was that something you did in addition to . . .

PS: Oh, no. That was, they did that part-time.

GC: That was part-time?

PS: They'd meet three times a year plus project site visits. It was a busy time. Plus they had to write the Summary. The Executive Secretary's job was to chair the Study Section meetings and to prepare a report, which is called the Pink Sheet. You've heard of the Pink Sheet?

GC: Sure, sure.

PS: Okay, and that would be one. That would form the document that we submitted to the Evaluation Council for review and approval.

[End Side A, Tape 2]

[Begin Side B, Tape 2]

PS: So that was the way that was done. I forget what point I was trying to make then?

GC: We were talking about the Pink Sheet.

PS: Oh, the Pink Sheets, yes, the Pink Sheets. I'm going to get a little bit off the topic now to discuss Pink Sheets.

GC: Okay.

PS: The Pink Sheets in the original form were an explanation of the action of the Study Section and usually were fairly short and concise. They were pretty much like this: "This is an excellent application from an individual with a substantial record in science in an institution which is very supportive of science, therefore we recommend approval." That was the sort of thing they wrote. And, on the contrary, "This is an application from an investigator who has had little experience or publication in his field and the institution in which he's working is not well known for research, and so on. Disapproval recommended." And, of course, the Council would usually go with it. It wasn't exactly a rubber stamp but it was *pretty* much. Unless there was a substantial reason for disagreeing with the Study Section, they would go along with it.

Well, the inevitable happened. Jim Shannon one day got a call from a congressman who was irate. "Dr. Shannon," he says. "I've just had a call from one of my constituents who had submitted an application to NIH and was disapproved and he wants to know why—and I want to know why, too." He asked, "Who was this? What's the institution?" and so forth. So he got that information. So, the phone rang on my desk and it was

Dr. Shannon. He said, "Palmer, I want to know why this application was disapproved. Why don't you send me the Pink Sheet and I'll look at it."

Well, the Study Section that had disapproved this was an Endocrinology Study Section and it was run by a real grizzly bear, Sam Hall. You'd have to know Sam Hall to appreciate him. Although Pink Sheets were fairly abbreviated, his were much more abbreviated than most. "The Study Section doesn't believe this thing should be approved. Disapproved."

GC: That was it?

PS: That's about it. Not much more. So, I looked at that and said, "Oh, my God!" So I called Sam up and I said, "Sam, I've got this Pink Sheet which you wrote for so and so and this is what's on it." "Yeah! What about it?" I said, "Well, what about it? Why was it disapproved?" "I told you. The Study Section didn't like it." So I said, "Is that the best we can say?" And I told him, "You know, Shannon wants it." "I can't help that. That's what the Study Section said. Tell him that." I didn't tell him that. I just sent the Pink Sheet over.

Well, just about the time the NIH mail reached Shannon's desk, the phone rings. "Palmer, I got this Pink Sheet. You know, it doesn't tell me a damn thing." I said, "No. I agree, Jim. It's not very explicit." He said, "Well, I'll tell you. You're in charge of this. What I want now is a Pink Sheet that's well constructed, that's got a statement of the problem, the rationale, why the Study Section is taking an action, point by point spell it out, and so forth." I said, "Jim, that's a long statement." He said, "That's what I want from now on!"

Now our Pink Sheets need to be three and four page documents. That became another major chore for the Executive Secretary because sometimes the Study Section members were never too explicit. A Study Section with maybe fifty applications, and the time between the end of the Study Section meeting and the Council review was relatively short. But an Executive Secretary had to prepare perhaps fifty Pink Sheets. So, we met with the Executive Secretaries and talked about this problem, and they suggested that we get the reviewers involved in this and have them write up the basis of their statements.

GC: Like write notes down?

PS: Yes. And then we could write a Pink Sheet based on that. So, that's what they started to do then, and that became sort of the norm for Pink Sheets. Of course, it didn't sit well with the reviewers, because it meant a lot more of their time, and so it got pretty elaborate. It got to the point where the reviewers had to prepare these before they even went on the project site visit. And the point was made that they didn't get paid expenses if they didn't have these on time. Oh, I mean, it got real gruesome. That was after I left DRG.

I think they have reformed it somewhat now, but it's no longer "This is a poor application from a bad institution. Disapproved." No longer that. That doesn't work any more. Well, anyway, that was the Pink Sheet. So, after the Allergy and Immunology Study Section . . .

GC: Okay. You went on to be the Associate Chief, Division of Research Grants, and you said that that was that they just changed it from a branch to a division.

PS: Yes.

GC: And then you were Deputy Scientific Director of Chemotherapy and that was under Gordon Zubrod.

PS: Yes. That was Dr. Gordon Zubrod.

GC: And that's when you first came to NCI.

PS: Yes.

GC: And it was Gordon Zubrod who brought you in to that position.

PS: Endicott.

GC: Endicott brought you in. And that's where you first met Gordon Zubrod then.

PS: Right.

GC: Okay. And then you were Associate Director for Extramural Activities.

PS: Yes.

GC: Which was a return to the kind of work you had been doing at NIH.

PS: Yes, yes.

GC: And then you were Director of the Division of Cancer Grants.

PS: Well, that's the same. It was a change of name. Yes, just a change of title.

GC: And then you were Director of the Division of Research, Resources and Centers.

PS: The same position. A change of title.

GC: So it changed twice.

PS: Yes.

GC: Okay. So, when you were at the NCI, I guess when you were working with Dr. Zubrod as Deputy Scientific Director, you were working mainly with Dr. Zubrod? Who else would you work with on a daily basis? If you can't remember names, just types of people. Would you go to the clinics at all?

PS: Some of the Intramural people. No, I didn't have much to do with the Clinical Center. It was, as I say, the only thing we had to with the clinical end was to review the clinical applications for testing drugs. That was mainly a job for review. I tried not to involve anybody in the Intramural program. I tried to set up my own little Extramural pocket in Gordon's program.

GC: Had there been much Extramural activity before you came at the NCI? Or did it really pick up when you got there?

PS: Oh, yes, there was a lot. Ralph Meador was the Associate Director when I was at DRG and he had been a very active Associate Director. He was a vigorous fighter for the Extramural Program, but he got beat down by Intramural folks. He was a mild-mannered man. He was an ex-professor of biology from Yale and a real gentleman. Poor fellow died a couple of years ago. He would have talked through ten tapes for you for this project.

GC: Really?! That's too bad that I didn't get to talk to him.

PS: Yes, yes.

GC: Calvin Baldwin was another person who mentioned your name. Is he someone you worked with on a . . .

PS: Yes. Calvin was an Executive Officer.

GC: An Executive Officer, right.

PS: So I worked with him, of course, on budget and things like that. Also a fellow member of the . . . we had an executive committee, of which I was a member and Cal was a member, and the other associate directors, like Zubrod and, I can't remember the names of the other people.

GC: He told me other names. Leo [F.] Buscher?

PS: No. Leo [F.] Buscher worked for me. Leo [F.] Buscher was in the management, financial management end. He was on my staff. He headed the financial affairs, budgets, and things like that. He's the one that prepared the budget so that we got enough money.

GC: Okay.

PS: If I had the telephone books, it would be easier, I could pick these other guys up.

GC: I should have brought them up with me.

PS: Yes.

GC: Those are the greatest things, to have those directories. They help so much.

PS: Yes, yes, that's right.

GC: It helps so much. I don't know if we've covered this already, but when I was talking to Dr. Zubrod, he said that Dr. Shannon created an internal panel when the Wooldridge Committee was criticizing . . .

PS: Yes. Wooldridge, yes.

GC: Is that the name you were trying to think of earlier?

PS: Yes, the Wooldridge Committee.

GC: And they divided the whole NCI into four divisions at that time. They restructured, and that's when you became head of the Grants Division.

PS: Yes.

GC: Did we pretty much cover what you wanted to say about that?

PS: Yes.

GC: Okay. We talked about the 1971 Cancer Act and you said that you really didn't have any input.

PS: Right.

GC: I was intrigued. You wrote an article in 1967 called "The Interests of the NCI and Comparative Pharmacology." What was that? I haven't read that yet, so I was just wondering what your conclusions were.

PS: We were doing a rather comparative physiology of drugs, and how they were different from animal to animal, and it was an attempt to get some work going in those areas. It wasn't a substantive article. It was sort of an editorial of mine.

GC: Was it easy to get published, coming from the Cancer Institute? Was that something that helped you get published or helped . . .

PS: Well, articles like that, of course, editorial-type, yes. But scientific articles, no, they would go through peer review. I never had any articles published by peer review when I was in NCI. That was not my area. I was doing something else.

GC: Right, right. Was the NCI known, how was the NCI known, what was its place in the scientific community while you worked there?

PS: It was not thought much of.

GC: Really?

PS: Everybody went to it because they had money, but they didn't think very highly of the Institute. Are you talking about the Intramural Program now?

GC: Either.

PS: Oh. It was sort of plus or minus. There was some positive in the fact that there was a lot of money available and it had some good people in the Intramural Program who were doing good work, like Zubrod and DeVita and people like that, although there were some people who questioned DeVita's work, but I can't speak to that, because I don't know. Ahh, what else? I guess I can't really say too much about that.

GC: It sounds like your job involved a lot of travel.

PS: Yes.

GC: Did you pretty much see every Cancer Center?

PS: Oh, yes! Saw it before and after.

GC: What would you do on the site visits? You'd meet with the . . .

PS: A site visit is different from visiting a campus. When we were promulgating the Cancer Center Program, we would mainly go there to try to sell the concept of a Cancer Center and sort of, if they wished to, to urge them to submit an application for a Cancer Center, and told them what the guidelines were and things like that.

GC: Okay. So that was a different purpose entirely.

PS: Yes. The site visit was different. A site visit was where you would take a group of scientists with you to actually review a proposal.

GC: Okay. And that's what the Study Section people did, is they traveled to go see these.

PS: Yes, that's right. So, being Executive Secretary in the early days, I went with every one of them, because the Executive Secretary had to be on all the site visits.

GC: Wow! So, how many weeks a year would you travel, would you say?

PS: Most of them were maybe two- or three-day visits. That's all. Weren't extended, sometimes we combined two or three visits in areas, but that was kind of rare. It didn't happen very often.

GC: Were you traveling most weeks of the year, would you say?

PS: I'd say at least once every two weeks. Sometimes more than that but at least that.

GC: That's pretty intense traveling.

PS: It was. And very interesting, actually. I met a lot of people.

GC: I bet!

PS: Yes.

GC: Did you do international visits, too?

PS: Yes. We were instrumental in funding the European Organization for Cancer Research, EOCR. We got them started, gave them some money, they still support them in a way. Oh, I just remembered somebody you should interview.

GC: Who's that?

PS: Bill Levin.

GC: Bill Levin?

PS: Bill Levin, L-e-v-i-n, who is now Emeritus Professor at the University of Texas Medical Branch. He has an office there and I'm sure he'd love to talk to you.

GC: Oh! That sounds great. So that would be another Extramural person for me to interview?

PS: Oh, yes, definitely. And another one would be Ross, R-o-s-s, McIntyre, the former Cancer Center Director at the University of Vermont. I think that's right. He's retired now, but he's still active and he's still going strong.

GC: He's still up in Vermont?

PS: Still, well, he retired to somewhere in Vermont, or maybe Maine. Somewhere in the New England area. Ross McIntyre.

GC: M-a-c?

PS: M-c-I-n-t-y-r-e.

GC: What are you proudest of in your work . . .

PS: Cancer Centers.

GC: The Cancer Centers?

PS: Yes.

GC: How do you feel when you see them now, when you see that there are still so many of them and that they're going?

PS: Well, kind of happy, I think, because if you want cancer treated, we always tell people to go to a place that has a Cancer Center, and that's the place where you *probably*, not necessarily, but probably will get the optimal treatment.

GC: Because of the approach, that multidisciplinary approach?

PS: The multidisciplinary approach, yes.

GC: What did you enjoy most about working there?

PS: Cancer Institute? I enjoyed the camaraderie with the University people and with my fellow people in the Cancer Institute. Just good relationships. I enjoyed meeting people who I only read about.

GC: Like Linus Pauling?

PS: Exciting.

GC: Was it easy to get money and resources and things like that to do the work you wanted to do?

PS: Work that I wanted to do?

GC: For the Extramural Programs and everything, was it pretty easy to . . .

PS: Oh, yes, yes. I didn't have any, I never had any budgetary problems. All the money I wanted, I got.

GC: Really!

PS: Within reason, of course. I remember talking about, we never had any budgetary problems, not while I was there.

GC: I have one more question about the 1971 Cancer Act. Part of the rhetoric at the time was that cancer was going to be cured in five years.

PS: Ten years, oh, was it five years?

GC: Yes. They said by 1976, by the bicentennial, cancer would be cured.

PS: Yes, yes.

GC: And I just wondered what you thought of that, or what other scientists thought of that.

PS: Nobody that I knew thought that was true or that it even came close to being true. It was just hoopla for publicity.

GC: And you were gone before 1976. You left in 1974.

PS: 1974, yes.

GC: I was just wondering how, when 1976 came around, if there was any reverberation of that rhetoric?

PS: Oh, no. Not at all. They're very cautious here. You don't see anything like that. Do you remember the War Against Cancer? That great War Against Cancer?

GC: Right.

PS: That sort of fizzled out.

GC: Did people think much of it at the time?

PS: I'm sure, yes, especially people who didn't know much about research and cancer. They thought, "Oh, this is going to be good. The more money you put in, the better chance you have of curing cancer. So, let's give them all the money they want!" Especially the Congress felt that way. Congress was always pushing the appropriation higher than what the President had asked for, and I think that continues pretty much to this day. So, we didn't have any problems. We had all the money that we needed, that could be used properly. There was no squandering of money that I know of. The money was used very circumspectly. Actually, as far as the Extramural Program was concerned, we spent no money unless it was recommended by a group of peers.

GC: Is there anything—I'm coming to the last of my questions—I was just wondering if there was anything I haven't asked you about or we haven't covered that you'd like to make sure that you add for the historical record.

PS: Well, I think I've interjected enough to cover most everything that I wanted to talk about. I wanted to talk about the war between the Intramural and the Extramural programs, which we lost, and it was one of those things. History is written by the victors.

GC: I just wanted to look at the part we had to tape over.

PS: As far as the Centers are concerned, it would be good to talk with some of the old-time Cancer Center Directors who experienced this all from the ground up. Ross McIntyre would be one man to talk to. Lee Clark, of course, is gone, so you can't talk to him. Sidney Farber's gone; can't talk to him. Frei. Have you got him on your list?

GC: I've interviewed him, yes.

PS: Okay. Emil Frei. And Freireich?

GC: Yes, I interviewed him.

PS: Okay.

GC: He's a character, too.

PS: Yes, he is. So, you've checked in with a lot of the people who were very active in the early days. You talked to Zubrod.

GC: Yes.

PS: Well, oh, I don't know, Gretchen, it's difficult to recall some of these names which were indelibly engraved on my brain, but apparently not so indelibly.

GC: You had started saying something earlier about Dr. Gallo, Bob Gallo, and his interest in the Extramural Program or . . .

PS: I think it's a good Intramural program. I'm not downgrading Dr. Gallo at all. But, because of the funding situation there, they were able to get a lot of the things that the normal investigator wouldn't be able to get—publications especially. And since most scientists live and breathe on their publications, the more publications you have, the better. You've got that much publication in your CV and somebody's got this much and, you know, they say, "Oh, this guy, look at all the work he's done. This other guy hasn't done any work!" Well, he might have done exactly the same work, but . . .

GC: Publications.

PS: Publications. That's the thing I think I wanted to talk about and I think we covered that pretty well. But, perhaps we put too much stock in the number of publications, and I think we do. We've attempted to pare down the applications by specifying peer review journals and, of course, that means different things to different people. Some journals are well reviewed. Some scientists may try to find the one that's not so well reviewed, so they get more publications, chance of getting a publication. That's not so easy to detect. I'm not accusing anybody of anything. It's just that it's human nature.

GC: Sure, sure. Well, I think I'll stop the tape.

PS: Okay.

GC: I think we've done a lot. I may come back and ask you some more questions, or call you or something.

PS: Okay. And when I get back to Galveston, which won't be until October, I will dig out some stuff which might be helpful to you in terms of the old Chemotherapy Program. I have a lot on that.

GC: That would be great.

PS: In fact, unfortunately, I threw my phone books away. But there may be other things that I have in the files. I've got two file cabinets full of stuff on NCI.

GC: Wow!

PS: Which I couldn't give to anybody when I left the lab. I wanted to give my phone books, but . . .

GC: They didn't want them?

PS: They said they had them, but I don't think they did. I checked later and they don't have them.

GC: I don't know who has them. That's too bad. That's really too bad.

PS: I said, "Well, this is fine archival material." They said, "What's archival?" Oh, my.

GC: I know, I know. Oh, I wanted to get you to spell a few names for me, to make sure I have them right. At the very beginning we were talking about Ernest Allen.

PS: Ernest, E-r-n-e-s-t. Allen, A-l-l-e-n.

GC: Okay.

PS: If he's still alive—he would be living in Bethesda, I think—he would be a good man to interview.

GC: Okay.

PS: Especially from an Extramural point of view. He probably has a lot more to say about this than I have and probably more accurately.

GC: I doubt that, but okay. Alfred Gilman? Is that right?

PS: Alfred Gilman is dead.

GC: Okay. But it's G-i-l-l-m-a-n?

PS: G-i-l-m-a-n.

GC: Just one "l."

PS: Of Goodman and Gilman fame. Famous textbook of pharmacology. Goodman's dead too, unfortunately.

GC: And Bill Levin was L-e-v-i-n?

PS: L-e-v-i-n, yes. William Levin. William C. Levin.

GC: Okay. One of the presidents, I think, down at the Medical School in Texas was something James?

PS: Thomas James, yes, Tommy. Tommy James, yes.

GC: Okay. Bill Walters?

PS: Bill Walter.

GC: No "s"?

PS: No "s." He's Welsh.

GC: I think I've got most of them. Wilkes?

PS: W-a-a-l-k-e-s. Philip.

GC: Philip Waalkes.

PS: I think he's retired.

GC: Okay. Do you know if it was one "l" or two "l's" in Philip? I can look it up. That's okay.

PS: I'm not sure One "l," I think, but I'm not sure.

GC: I'll look it up. Bert Cole. Was that B-u-r-t?

PS: Robert Cole.

GC: Robert. So it's B-e-r-t.

PS: Yes. B-e-r-t and C-o-l-e.

GC: I had spelled them both wrong. Okay. Leon Schmidt?

PS: Leon Schmidt is Leon and then Schmidt, S-c-h-m-i-d-t.

GC: Okay. And I always get mixed up on Stu Sessoms, Stuart Sessoms. Is it S-t-e-w?

PS: S-t-u-a-r-t.

GC: And he would go by "Stu"?

PS: Yes.

GC: Sam Hall was just Sam Hall?

PS: Sam Hall, Samuel Hall.

GC: Okay. Okay?

PS: Okay.

GC: All right. This ends the interview.

[End of Interview]